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THE STIMULUS-NEURAL CONTROL OF BEHAVIOR DURING AND AFTER LEARNING[1]

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IN the present paper I wish to discuss two classical psychological theories dealing with certain aspects of the learning process and to evaluate them in terms of experimentation. The first of these theories is to the effect that "consciousness" gradually disappears during learning, so that the completely learned response is made automatically and "unconsciously." An individual, for example, is said to be acutely aware of his piano playing in the beginning of the formation of this habit; but as learning proceeds, the playing is done more and more automatically until finally the subject can play all but "unconsciously," while giving his attention to other things and while simultaneously engaging in other activities. The second theory that I wish to discuss also deals with changes that are alleged to go on during the process of habit-formation. If we may continue to use the illustration of piano playing, this theory holds that originally such a serial action is controlled partly by visual and auditory stimuli, but that after playing has become perfected the stimulus control is turned over to proprioception. At this stage the stimuli from one arm-finger movement are adequate to produce the next arm-finger movement which produces new proprioceptive stimuli to arouse the next response, and so on. Thus the theory holds that the control of a perfected serial action is proprioceptive and that the process of the formation of such a habit involves, among other things, a shift from exteroceptive to proprioceptive control, making possible the automatic character of the final response. We shall first discuss the theory

[1] Address of the vice-president and chairman of Section I—Psychology, American Association for the Advancement of Science, Boston, December 29, 1933.

of shifting stimulus control of serial action and then turn to a brief analysis of the view that perfected habits are executed "unconsciously."

The theory that serial action is of the chained reflex type and that during learning the stimulus control shifts from the exteroceptors to the proprioceptors can be most fruitfully discussed on the basis of experimentation with the maze. The theory, however, was well established in American psychology before Watson, in 1907, wrote his monograph on the rôle of kinesthetic and organic sensations in the maze behavior of the rat.[2] The chief historical influence in establishing this theory was undoubtedly exercised by James in his chapter on "habit," a chapter that contains the still current diagram of the mechanics of serial action. The experiments reported in Watson's monograph involved the capacity of the white rat to learn the maze when deprived of various receptors, and the general conclusion drawn was that only proprioception was necessary for the learning and mastery of the maze habit, which was described as "a serially chained kinesthetic arc system." In 1914 Watson[3] held that the paper on orientation in the rat which he and Carr[4] had published offered "very definite and positive proof" that the above thesis was correct. And Washburn,[5] in 1926, endorsed the same theory on much the same evidence.

I shall not in the present paper again review the literature on the stimulus-neural control of the maze habit. It is necessary, however, to refer to certain studies in order further to clarify the problem of the possible shift of stimulus control during learning. In 1915 Vincent[6] published some important experiments, in which it was shown that the rat could utilize outstanding visual and olfactory stimuli during the learning of the maze. Animals that learned a black-white maze or an olfactory-trail maze were more irregular in the speed of their perfected responses than animals that learned the normal maze. This relative lack of automaticity in the responses Vincent attributed to the distractive influence of the olfactory and visual stimuli, respectively, the implication being that proprioceptively controlled responses would be more automatic than those controlled by other receptors. Vincent wrote as follows: "After the problem is learned, in the slow turning over to kinesthesis, when attention is freed, these [visual] sensory factors may still retain their potency in times of momentary distraction" (p. 24). After the above experiments were completed and the mazes learned, Vincent transferred the animals to olfactory and visual discrimination boxes in order to determine whether or not olfaction and vision were controlling the perfected maze responses or whether the control had shifted to kinesthesis. In both cases the results suggest that the exteroceptive senses were still involved in the control of behavior. There was no indication of a shift in stimulus control. In one experiment in the olfactory-trail maze, Vincent removed the trail after the maze had been mastered with the trail present. Under the changed conditions, the rats still ran perfectly. This, however, does not prove that the stimulus control of the behavior has been turned over to kinesthesis. It indicates rather that whatever stimuli were present after the olfactory trail was removed were capable of controlling a correct maze run.

The work by Dennis and Porter[7] (1932) clearly supports such a conclusion. They trained rats to run from the center of a circular platform to the periphery in order to secure food which was always located just beyond and below the end of a narrow metal strip running from center to periphery of the platform. A white square cardboard was placed at the food and within the rat's field of vision from the start of each run. The results show that when the card was removed, the metal strip controlled the rat's response and *vice versa*. In other words, there was a multiple stimulus control, even of the perfected response; and in the absence of a part of this complex stimulus, the remaining parts controlled the behavior.

In 1917 Carr[8] wrote "that the white rat learns the standard maze in tactual and kinaesthetic terms [and] that during the learning the control is gradually transferred from contact to kinaesthesis . . ." (p. 259). Carr's own extensive experiments were directed toward the analysis of the dependence of the maze habit upon the rat's total sensory environment. The results led him to conclude, rightly, I think, that "This act is dependent—both during and subsequent to its development upon a wider sensory situation of which it is a part" (p. 304). The two quotations from Carr are apparently contradictory. The first is partly a restatement of the psychological tradition that there is a shift in stimulus control during habit-formation and partly a conclusion based, probably, on the work of Bogardus and Henke[9] (1911) where it was found that normal (seeing) rats made many contacts with the

[2] J. B. Watson, "Kinesthetic and Organic Sensations," *Psychol. Rev.*, Monog. Suppl., 8, No. 33, 1907.

[3] J. B. Watson, "Behavior." New York: Holt, 1914.

[4] H. Carr and J. B. Watson, "Orientation in the White Rat," *Jour. Comp. Neur. and Psychol.*, 18: 27–44, 1908.

[5] M. F. Washburn, "The Animal Mind." 3rd ed. New York: Macmillan, 1926.

[6] S. B. Vincent, "White Rats and the Maze Problem," *Jour. Animal Behav.*, 5, 1915.

[7] W. Dennis and J. M. Porter, Jr., "Isolated Action of Compound Stimuli in a Locomotor Habit of Rats," *Jour. Genet. Psychol.*, 41: 127–135, 1932.

[8] H. A. Carr, "Maze Studies with the White Rat," I, II, III. *Jour. Animal Behav.*, 7, 259–306, 1917.

[9] E. S. Bogardus and F. G. Henke, "Experiments on Tactual Sensation in the Rat," *Jour. Animal Behav.*, 1: 125–137, 1911.

corners of the maze during learning, but later eliminated these. However, the more recent work by Dennis[10] (1929) indicates that while normal rats may make this change in behavior blind rats do not. The suggestion is, therefore, that vision played a rôle in Bogardus and Henke's experiments, and that kinesthesis was not alone involved in the completed habit.

Since 1917 many studies have appeared concerning the possible rôle of kinesthesis in the control of the perfected maze response. Rats have been trained to run a maze and then have been forced to swim the same maze, which they have done correctly in spite of the changed proprioceptive stimulations (Macfarlane[11]). Rats have been crippled either by muscular or neural operations and have still run the maze correctly (Dorcus and Gray;[12] Lashley and McCarthy[13]). Lashley and Ball[14] have interfered extensively with the spinal conduction paths, and still the rats could run the maze. Hunter[15] and Casper[16] have used mazes in which proprioceptive factors could hardly function to control a perfect response, and yet the rats learned the maze. Furthermore, Walton[17] has failed to find evidence that rats who form a visual maze habit come to depend upon proprioception. There seems to be no question but that kinesthetic cues are not necessary for the execution of such a serial response. *However, there is a great danger, already apparent in the literature, that the additional and unjustified conclusion will be drawn to the effect that proprioception is not normally involved in the control of serial action.* All that the evidence indicates is that, in some cases and perhaps in many more, proprioception can be altered or made to offer subliminal differences without disruption of the serial response. In such cases the evidence is that nonproprioceptive organs function as response controls. In the usual maze behavior and in such serial action as walking, as well as elsewhere, proprioception may occupy an important place both during and after learning.

We may, therefore, summarize the situation as follows: (1) No single receptor system is necessary for learning the usual maze habit or for executing it after mastery; (2) vision, olfaction, touch and proprioception are usually active in the control of the perfected maze habit, where experimental conditions permit; (3) there is thus no evidence that the control of this serial response is handed over to proprioception when and as learning is completed; and (4) if exteroceptors and proprioceptors are involved in the execution of the completed habit, there is no obvious reason for assuming that they were not involved earlier during the formation of the habit.

Some additional evidence on the problem of the shift in stimulus control has been gained by other than maze methods. While Watson was at work at the University of Chicago on the nature of the sensory control of the maze habit, the late June Downey was investigating, in the same laboratory, the control processes in handwriting. Downey[18] studied such highly automatized and over-learned responses as the signing of one's own name and other writing habits not so well established. Tests made with the subject blindfolded indicated that vision was partially effective in the control of the response. The nearest approach to evidence that the kinesthetic processes from the moving hand were sufficient to control the writing habit comes from certain cases where, with an effective distraction, the subject was still able to write correctly for several seconds, although even here the shaping of the various letters reveals the inadequacy of the response under such conditions. The general result of Downey's study was to show the complexity of the control processes for writing, with visual, auditory and vocal-kinesthetic factors operating. It was probably as a result of this work that Angell, when he came to discuss James' diagram and the conception of chain-reflex proprioceptive control of habit in his "Psychology" (1908), wrote as follows: "This description is probably roughly correct, but it presents far too simple and diagrammatic a picture to be taken literally. Experiments show that in such cases as writing illustrates, the sensory cues which are involved come from the eye and even the ear quite as often as from the muscles and joints" (p. 72).

The studies of typewriting which Book[19] performed at Clark University also give us some information on the stimulus control of serial action, although they were not primarily directed to this problem. The subjects who used the touch system of typing received no

[10] W. Dennis, "The Sensory Control of the White Rat in the Maze Habit," *Jour. Genet. Psychol.*, 36: 59–90, 1929.

[11] D. A. Macfarlane, "The Rôle of Kinaesthesis in Maze Learning," Univ. Calif. Publ. Psychol., 4: 277–305, 1930.

[12] R. M. Dorcus and W. L. Gray, "The Rôle of Kinaesthesis in Retention by Rats," *Jour. Comp. Psychol.*, 13: 447–451, 1932.

[13] K. S. Lashley and D. A. McCarthy, "The Survival of the Maze Habit after Cerebellar Injuries," *Jour. Comp. Psychol.*, 6: 423–433, 1926.

[14] K. S. Lashley and J. Ball, "Spinal Conduction and Kinaesthetic Sensitivity in the Maze Habit," *Jour. Comp. Psychol.*, 9: 71–106, 1929.

[15] W. S. Hunter, "A Further Consideration of the Sensory Control of the Maze Habit in the White Rat," *Jour. Genet. Psychol.*, 38: 3–19, 1930.

[16] B. Casper, "The Normal Sensory Control of the Perfected Double-alternation Spatial Maze Habit of the Albino Rat," *Jour. Genet. Psychol.*, 43 (in press).

[17] A. Walton, "Visual Cues in Maze Running by the Albino Rat," *Jour. Genet. Psychol.*, 38: 50–77, 1930.

[18] J. E. Downey, "Control Processes in Modified Handwriting," *Psychol. Rev.*, Monog. Suppl., 9: No. 37, 1908.

[19] W. F. Book, "Psychology of Skill," Univ. Montana Bull., 1908. (Also, New York: Gregg, 1925.)

direct visual cues from their hands, but they did read visually from copy and they may have received some visual stimulation in a peripheral manner from their hands. Along with the visual reading went, even in the expert stage, "incipient or actual pronunciation of the words, itself a form of the group 'spelling,' operative in the previous (word association) stage, that now initiated and directed the letter-making movements. The incipient or actual pronunciation of the words somehow directed and controlled the sequence of the letter-making movements" (p. 57). And again Book writes, this time in connection with the work by the sight system: "It was determined that a sort of half conscious incipient mental spelling occurred and was required for months after it seemed [to the subjects, as their reports indicate] that all traces of the spelling had disappeared" (p. 87). So far as Book's attitude toward the relation of "consciousness" and the performance of a highly perfected habit is concerned, we may quote the following: "As has already been pointed out in preceding sections, these processes need a minimum of oversight for a long time even after they seem completely self-regulative. In other words habits are perfected or sink to the realm of the unconscious very gradually" (p. 135). I shall come back later to a discussion of the "unconsciousness" of perfected habits; but at present I wish to offer a further analysis of the problem of shifting stimulus control, based upon suggestions derived from experiments on non-serial action.

In non-serial action of the conditioned response type a non-effective stimulus becomes effective through being paired, under certain conditions, with another stimulus which will elicit the given response. Thus if a sound is paired with shock under certain conditions, the sound will come to arouse the response (for example, finger withdrawal) which was formerly made only to the shock. Overtraining the subject does not result in a shift of sensory control from the sound to something else. According to Pavlov, the neural control involves the cerebral cortex, and so far as we know no amount of overtraining will switch the neural control to subcortical levels. If the non-serial action to be considered is of the type found in the usual visual discrimination experiment, analysis will show again that there is no shift in the stimulus control of the correct response, no matter how long the training continues. The experiment is so planned that visual stimuli will determine to which side of the apparatus the animal will go in order to escape and secure food. Once this response is connected with the light, it remains so connected irrespective of overtraining just so long as the experimental conditions are unchanged. The neural impulses set up by the light pass through the occipital cortex, and Lashley's work indicates that overtraining does not reduce them to a sub-cortical level, although his further work shows that the subcortical areas may function adequately for this behavior, if the animal is trained after loss of the cortical areas. Thus so far as non-serial action is concerned, training brings no shift in the stimulus-neural control of the correct response. One begins with a stimulus—neural-process—response and one ends the experiment with the same threefold phenomenon.

The analysis of the experimental literature on the maze habit shows that no shift in the stimulus control of this behavior during training has been found. Theoretically this is what would be expected. The rat in the maze will be affected by many stimuli both within and without the maze, both proprioceptive and exteroceptive stimuli. Certain of these stimuli will be sufficiently constant in relation to the correct pathway through the maze to serve as controls, or cues, for the maze-running behavior. It is true that, as the subject eliminates errors, certain responses no longer appear, and the stimuli for these responses no longer have their former effectiveness. However, the theory of the shift in stimulus control during learning does not refer merely to the fact that, when certain responses are dropped and certain others appear, a shift in stimulus control is usually involved. *The theory is that what are called the correct responses in the maze have first, let us say, a visual-olfactory-proprioceptive control whereas with additional training of the subject this control becomes solely proprioceptive.* For such a theory there is no experimental evidence. The only reasons why such a shift would appear, if it did appear, are (1) that proprioception is more invariably connected with the behavior of running the true path than are the other stimuli and hence with training it is favored more and more at their expense and (2) that proprioceptive stimulation is of a sufficiently greater prepotency to insure it the right of way over visual and olfactory stimulation.

If we are not justified in making the generalization that the learning of serial action necessarily involves a shift in stimulus control from exteroceptive to proprioceptive stimulation, neither are we justified in making a general denial that proprioception may at times be essentially involved in the control of completed habits. If exteroceptive forms of stimulation vary too much, or too little, to afford means of control, proprioception may carry the burden alone. If proprioception varies too greatly, or too little, then the control of the behavior is effected by the remaining forms of stimulation. Thus whether or not, and to what extent, proprioception controls the perfected serial action depends upon the particular situation in

which the responses are manifested and not upon a general law of the shift in stimulus control.

Let us turn now to a very brief analysis of the psychological theory that "consciousness" lapses during the process of learning and is absent from automatic acts. I introduce a discussion of this theory in the present context because of the implication in the theory that the stimulus-neural control of behavior changes during learning and over-learning in such a way that the subject's report (introspection) can no longer be elicited. We have already seen experimental reasons for doubting the existence of any significant changes in the character of the sensory-neural control of behavior as the automatic stage of performance is reached. Let us now see how defenders of the "lapsed consciousness" theory formulate their position. Wundt writes as follows in his "Outlines of Psychology" (1907): "This gradual *reduction of volitional to mechanical processes,* which depends essentially on the elimination of all psychical elements between the beginning and end of the act, may take place either in the case of movements which were originally impulsive, or in the case of movements which have become impulsive through the retrogradation of voluntary acts. It is not improbable that all the reflex movements of both animals and men originate in this way" (p. 214). Titchener writes in his "Textbook of Psychology" (1910) as follows: "The author, then, believes, with Wundt and Ward and Cope, that the earliest movements were conscious movements, and that all the unconscious movements of the human organism, even the automatic movement of heart and intestines, are the descendants of past conscious movements" (p. 452). In elaborating on the changes which action undergoes, Titchener says that "there is a tendency toward the simplification of movement, and the realization of this tendency is accompanied by lapse of consciousness" (p. 456). Angell's position in his "Psychology" (1908) can be indicated as follows: ". . . consciousness occupies a curious middle-ground between hereditary reflex and automatic activities upon the one hand and acquired habitual activities upon the other." When hereditary modes of adjustment fail to adapt the organism to its environment, consciousness immediately appears and "enters upon its characteristic cycle. At first of course its activities are vague and crude. But presently we find selected from out the masses of motor responses created by the sensory stimulations to which the sense organs are sensitive, those particular ones which issue in effective muscular control over the environment, and straightaway we are confronted with habits. As soon as these habits are firmly established, consciousness betakes itself elsewhere to points where habitual accommodatory movements are as yet wanted and needed" (p. 74–75). And again in his "Introduction to Psychology" (1918) Angell writes: "When [habits] have become sufficiently perfect, they are passed over almost entirely to the automatic control of the nervous system, leaving the mind itself free to go forward to the creation of other habits . . ." (p. 51). Similar quotations could be taken from the writings of many eminent psychologists, but the statements above given will suffice to indicate the general nature of the classical theory.

In support of the "lapsed consciousness" theory, or as a result of the theory, there is still a wide-spread tendency to comment upon the influence of training as a factor in permitting the organism to engage simultaneously in two or more activities. So long as both acts involve consciousness, their simultaneous activity is said to be difficult or impossible; but when habit has reduced one, or both, to the unconscious level, their simultaneous execution is possible. There is here a very genuine problem in the determination of the conditions under which two or more activities can be carried on by the subject; but the solution of the problem will not come, as it has not come, from speculations on the relation of attention and consciousness to habit formation. Rather the solution will result from an analysis of the stimulus—neural-process—response conditions which are necessary to permit the simultaneous and non-interfering occurrence of two forms of behavior. When such an analysis is experimentally made, it will reveal, I think, that the phenomenon is due to the independent organization of the two (or more) coordinations and not to any general factors of "lapse of consciousness" and "proprioceptive control" of serial action.

Oddly enough, there is little or no experimental support for the "lapsed consciousness" theory, although attempts have been made to interpret studies like those on reaction time in such a manner as to provide a basis for the theory. The mainstay of the theory is anecdotal evidence; and yet it would be relatively simple to study the problem in so far as such a problem can be formulated in scientific terms. Whatever differences may exist between mentalistic psychologists and behaviorists with reference to the existence and nature of consciousness, the fact is generally admitted that the only objective evidence of the presence of "consciousness" is the subject's report, usually verbal. The behaviorist, then, might state the problem as follows: How does the subject's report vary with the degree of training on any given response or series of responses? Since the subject's report is the behavior from which the mentalistic psychologist infers the existence of specific "consciousness," the lapse of "consciousness" should be indicated by the subject's inability to report during the final stages of habit

formation and after the responses have become automatic. So far as I know, no reports of such experiments have ever been published.

Some years ago I conducted a preliminary series of experiments on this problem, and the results are here outlined (briefly and rather inadequately) for the first time. In the first experiment the subject was instructed to name the objects that he saw. He was then shown tachistoscopically, exposure time 1/5 sec., a card on which were six colored squares and six lines. After each presentation of these stimuli, the subject was asked to name what he had seen. During the training his responses become more and more accurate until perfection was reached and the habit was established of responding to certain visual stimuli which were always the same. The subject was then greatly overtrained until both in time and accuracy his responses varied but little, *i.e.*, until the stage of automaticity was well established. After a given presentation and response the subject was then asked, "Did you see red?" (one of the colors shown) or "Did you say red?" (a response that he had made). In practically every case the subject reported correctly. Perhaps this was due to the fact that the same stimulus card was always used and, while the subject really could not remember what he had seen or said, he thought that he must have seen and said the usual thing. Therefore, certain control tests were made where a stimulus card was used which differed in some one particular from the usual card, let us say that the color red was changed to blue. When the new card was presented the subject immediately said "blue" in place of "red," although he did not know in advance that a new card was to be used. Apparently the visual stimuli were still effective and the control of the response had not been turned over to proprioception.

In a second experiment other subjects were to repeat a passage of prose first spoken by the experimenter. Trial after trial this was repeated until the subject was not only perfect but greatly overtrained. During the entire course of the experiment, the subject, during the time that he was reciting the prose passage, traced a star-outline under mirror drawing conditions. His instructions were to repeat exactly what the experimenter said and at the same time to work for speed and accuracy in the star tracing. After 100 trials the subject was tested, without warning, by changing some word in the prose passage and at other times by being asked to report what he had said. The results indicate that the subject was able to report with a high degree of accuracy, and that the words spoken by the experimenter in reciting the passage still controlled the subject's behavior.

A third experiment was then devised in which a greater premium would be placed on the accessory task. New subjects were used, and they too repeated a prose passage after the experimenter had finished reciting it. While listening to the experimenter and while reciting, the subjects performed the following tasks: They traced with a metal stylus a narrow black line which moved in a zigzag manner behind a slit. Whenever the stylus left the line, the subject received a slight electric shock through the stylus. Along the line and at varying intervals were certain numbers from one to five, each number corresponding to a digit of the left hand. The subject was to signal the appearance of each number by pressing a key with the appropriate digit. Ten dollars reward was posted for the best performance on this tracing-signaling problem in the hope that this would, for the subject, become the major problem and the recitation the minor one. After the 100th trial slight changes were occasionally introduced into the prose passage and the subject's recitation noted, after which he might be asked what he had said. In this experiment again, training brought no decrease in the subject's ability to report, and it brought no detected shift in the stimulus control from the auditory words of the experimenter to the proprioception of the subject.

It may be said that the training described in the above experiments was not continued long enough to bring about a "lapse of consciousness." Undoubtedly studies based upon more extended training are desirable, and yet it should be noted that the above experiments extended well beyond the beginning of automaticity of the response.[20] The general relationship between training and report which one would expect depends upon the nature of the instructions and is as follows: (1) If the subject is instructed to report his own movements, there will be a decreasing quantity of report during learning which will roughly parallel the actual decrease in quantity of response made by the subject. There is as yet no evidence that the subject would ever be unable to report his

[20] It should be noted that the theories of "proprioceptive control" and of "lapsed consciousness" deal primarily with automatic behavior without a clear definition of the term automatic. Unless one utilizes anthropomorphic implications of the absence of volition, automatic behavior is identified by its relative constancy, from trial to trial, in such characteristics as speed, accuracy and work accomplished. Such automatic behavior is found not only when the learning curve has reached the final level of attainment but also during the periods known as plateaus. Furthermore, all final levels of attainment are in reality incompleted plateaus to the extent that they are above the physiological limits of behavior and to the extent that further training of the subject may bring a change of behavior more nearly approximating the physiological limit. Therefore any theory that claims the presence of certain phenomena during the period of automaticity must make clear what period of automaticity is meant.

behavior, no matter how long he is trained, and thus there is no evidence for the psychologist to use as a basis for the inference that "consciousness" finally disappears. (2) If the subject is instructed to report not upon his movements but upon the stimuli which are presented unchanged from trial to trial, his report will either be accurate from the beginning of learning or it will increase in accuracy with training. There is no evidence that the subject will drop from accuracy to inaccuracy or from report to no report with overtraining, unless factors like fatigue are introduced.

These experiments, limited and inadequate as they are, nevertheless point the way toward future work which will more fully describe the relation between the subject's report and his degree of training in some specific response. I have said nothing in this discussion about such abnormal phenomena as dissociated personalities and automatic writing, because the theory of "lapsed consciousness" is a *general* theory of the relation of "consciousness" to learning. Experiment would undoubtedly confirm what anecdote has reported, to wit, the existence of some forms of behavior which were once reportable by the subject but which have ceased to be so as a result of some process of reorganization within the individual. This specific field of application for the theory would undoubtedly well reward the investigator bold enough and careful enough to till it!

For the present we have arrived at a general conclusion concerning the stimulus-neural control of behavior during and after learning to the effect that once stimuli and responses have been connected no amount of overtraining, under constant experimental conditions, will result in a necessary shift of the stimulus-neural control. This is the conclusion which would have been anticipated by investigators had they not been under the influence of the two psychological theories which we have discussed, one that "consciousness" lapses when the automatic stage of performance is reached and the other that the stimulus control of perfected serial action is proprioceptive in kind. If and to the extent that a shift in stimulus control occurs, the causal factors will probably be found to lie in the greater constancy of one form of stimulation or in its greater prepotency rather than in a general law that such a shift is inevitable and generally to be expected.

THE BIOLOGY OF HEAVY WATER

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As soon as it was found possible to prepare, with any desired degree of purity,[1] a new kind of water in which ordinary hydrogen, H^1, is replaced by its heavy isotope, H^2, it was interesting to ascertain what effect this heavy water would have upon living organisms. Several months ago the experiments were interrupted, and since there may be no immediate opportunity of resuming them it seems best to publish the somewhat sporadic results so far obtained.

On account of the very small amount of pure H^2_2O available it was necessary to begin with small organisms. The first experiments dealt with the germination of tobacco seeds and a part of the results have already been published.[2] These seeds, which in ordinary water infallibly germinated within two days at 25° C., did not germinate at all in pure H^2_2O, as far as could be seen macroscopically. On the other hand, in water containing 50 per cent. H^2_2O the seeds all germinated and developed about half as fast as in ordinary water. Later this experiment was carried on for one month, and although the development continued to be slow, the seedlings appeared to be perfectly healthy and normal.

The tobacco seeds which had remained three weeks in pure H^2_2O without germinating, were then placed in ordinary water. At first it appeared that their power of germination had been completely destroyed, but after a week half of the seeds began to sprout, although in an abnormal way. The sprouts were extremely thin, and this sickly growth came to an end after a few weeks. There was no possibility of toxic impurities in the heavy water, as it had been very carefully distilled in a high vacuum at a temperature in the neighborhood of the freezing point. There is a remote possibility that some hostile organisms had been favored by the treatment with heavy water, but while this might conceivably explain the unhealthy growth of the seedlings, it could hardly account for the fact that one half the seeds did not germinate at all.

I believe that this whole phenomenon can best be explained by assuming that when the heavy water first began to permeate the seeds, it produced, together with the "bound water" already in the seeds, a medium in which germination could begin, but that as soon as the total water within the seed reached a

[1] Lewis, *Jour. Am. Chem. Soc.*, 55: 1297, 1933; Lewis and Macdonald, *Jour. Chem. Phys.*, 1: 341, 1933; Lewis and Macdonald, *Jour. Am. Chem. Soc.*, 55: 3057, 1933.

[2] Lewis, *Jour. Am. Chem. Soc.*, 55: 3503, 1933.

high concentration in heavy hydrogen, the process of germination was stopped. It seems reasonable to believe that it was this inhibition of the process of germination, once begun, that proved lethal, immediately to some, and ultimately to all of the seeds. If this assumption is correct, it should be possible to saturate seeds with heavy water without destroying their power to germinate. If seeds which germinate only at a higher temperature were soaked in pure H^2_2O at about the freezing point, then kept in this medium for several weeks and finally washed for some time, at the freezing point, in ordinary water, their power to germinate might prove to be undiminished.

A few inadequate experiments were then tried with micro-organisms. A set of tubes, filled alternately with ordinary water and pure H^2_2O, both containing 5 per cent. of malt sugar with small amounts of necessary inorganic salts, were inoculated with a pure yeast culture. In a similar set of tubes "shotgun" inoculations were made with traces of common dust. In both sets the marked difference between the two solutions could be seen with the naked eye. The solutions in ordinary water became cloudy, and in the second set soon became covered with mold, but the solutions in pure H^2_2O at the end of several days remained transparent and seemed, under the microscope, to be entirely sterile. The experiments were interrupted at this point, but after two weeks mold was observed upon one of the solutions containing heavy water. This one observation can not, however, be taken as proving that organisms may develop in very high concentrations of H^2_2O. The tubes were protected from the air only by cotton plugs, and certainly there must be some interchange between the water inside and the moisture of the air.

Pacsu[3] has recently made quantitative measurements of the rate of evolution of carbon dioxide from glucose solutions containing 4 to 6 per cent. yeast in ordinary water and in H^2_2O. The rate of fermentation proved to be about ten times as great in the former as in the latter. Since the rate was highest at the beginning and diminished during the experiments, and since relatively large quantities of yeast were employed, the rate of fermentation was presumably determined, not by the growth of the organism, but by the large amount of enzyme initially present.

It must be noted that in my own experiments and in those of Pacsu the solvent was never pure H^2_2O; and even if the solvent were introduced in the pure state and there were no interchange with water on the surface of the container or with the moisture of the air, still there must be an immediate interchange of hydrogen between the solvent and, presumably, at least half of the hydrogen of the sugar. In all the experiments, therefore, at least several per cent. of ordinary water must have been present. I believe that it will be found that no organism will grow in pure H^2_2O. However, in order to make a conclusive demonstration it will be necessary to dissolve the nutrient substances in pure H^2_2O, then to evaporate to dryness and add a fresh quantity of pure H^2_2O, protecting the solution at all times from contamination by water adsorbed on the tube and by the moisture of the air.

It next seemed desirable to try higher organisms, and because of their size and docility, flatworms (*Planaria maculata*) were selected. These, when placed in water containing over 90 per cent. of H^2_2O, soon began to lose their activity and within an hour or two they had released their hold upon the containing vessel and appeared to be dead. After remaining in the heavy water for four hours they were washed and placed in ordinary water. After a lapse of several hours some of them came slowly to life and ultimately a little more than half had resumed their normal activities, but the others were dead. Since this experiment was tried, work with the same organism has been reported by Taylor, Swingle, Eyring and Frost,[4] who state that flatworms were destroyed within three hours in 92 per cent. heavy water. The same authors also studied tadpoles, small fish and a certain protozoan, all of which were reported killed by 92 per cent. heavy water in periods ranging from one to forty-eight hours. They found, however, no lethal action of water containing 30 per cent. of H^2_2O.

Finally, I wished to test the effect of heavy water upon a warm-blooded animal. For this purpose I obtained three young white mice of respectable ancestry, weighing approximately ten grams apiece, and kept them in the laboratory for several days while their normal habits were being observed. Then, after they had all been deprived of water over night, two of the mice were given ordinary water while the third was given heavy water, administered by means of a medicine dropper, with a screw clamp upon the bulb which permitted the water to be forced out slowly and steadily. In this way the mouse drank all the water administered without losing a drop. Nevertheless, the experiment was a very costly one and I regret that since it was undertaken solely to ascertain whether the heavy water would be lethal, no preparation was made for a careful clinical study of the effects produced.

The answer to the main question was decisive. During the course of three hours the mouse received,

[3] *Jour. Am. Chem. Soc.*, 56: 245, 1934.

[4] *Jour. Chem. Phys.*, 1: 751, 1933.

in three doses, a total of 0.54 g of 87 per cent. and 0.26 g of 71 per cent. heavy water, containing altogether 0.66 g of pure H^2_2O. This would be equivalent, weight for weight, to a consumption of 4 or 5 liters of heavy water by an adult human being. The mouse survived and on the following day and thereafter seemed perfectly normal. Nevertheless, during the experiment he showed marked signs of intoxication. While the control mice spent their time eating and sleeping, he did neither, but became very active, running and leaping about and spending much of the time, for some mysterious reason, in licking the glass walls of his cage. The more he drank of the heavy water the thirstier he became, and would probably have drunk much more if our supply of heavy water had not given out. The symptoms of distress that he showed seemed more pronounced after each dose but not cumulative with succeeding doses, which leads me to suspect that the heavy water was being rapidly eliminated by the mouse. This could have been ascertained if suitable preparation had been made.

When we consider all these experiments we may conclude that heavy water is never toxic to any high degree and that it is tolerated in high concentrations by lower organisms. In such cases the rate of the vital processes seems to be roughly proportional to the fraction of the total hydrogen which is H^1. When all the H^1 is replaced by H^2, growth is certainly extremely slow and is probably entirely inhibited. When we seek an explanation of these phenomena there is no question but that it is to be found in the greatly reduced rate of all physico-chemical processes when H^2 is substituted for H^1. This is seen in the lower mobility of the heavy hydrogen ion,[5] in its much lower rate of deposition at a cathode, in the diminished rate of mutarotation of sugars[6] containing heavy hydrogen, and in the fermentation experiments to which reference has been made. Professor Rollefson in this laboratory is studying a photo-chemical reaction in which H^1_2 reacts thirteen times as fast as H^2_2. Now it seems likely that in the complicated chain of processes which are necessary to growth, there are some whose rate is so enormously decreased by the substitution of H^2 for H^1 that they are practically inhibited. The inhibition of a few essential processes would inhibit all the processes which must run concurrently, or in sequence. Thus in a system containing no ordinary hydrogen vital growth would be suspended, while in one containing both H^1 and H^2 the process of growth would be approximately proportional to the fraction of the total hydrogen which is H^1.

One of the first experiments that should be tried is to grow some organism for a considerable period of time in a mixture of the two kinds of water, and then by analysis of the dried tissues to find whether the two isotopes are used in the proportion in which they exist in the water, or whether there are mechanisms which permit the preferential employment of H^1, or even in some cases of H^2. It is not inconceivable that heavy hydrogen, which exists in small amounts in all natural water, may actually be essential to some plants or animals. A supply of water almost completely freed from the heavy isotope is now being prepared for the purpose of conducting such studies.

SCIENTIFIC EVENTS

THE EMERGENCY COMMITTEE IN AID OF DISPLACED GERMAN SCHOLARS

THE Emergency Committee in Aid of Displaced German Scholars has submitted its annual report. It is pointed out that the procedure followed by the committee has been simple. Due to the response of so many universities, colleges and institutes, and due also to the limited funds at its disposal, it was found necessary to make grants in preferential order to a selected list of institutions extending from the Atlantic to the Pacific oceans. At first, grants were made for a single scholar in each institution. Later, in isolated cases, a second grant was made. Funds have been made available directly to the administrative heads of institutions.

The Emergency Committee records with satisfaction the monetary assistance received from foundations, especially the New York Foundation and the Nathan Hofheimer Foundation; from the American Jewish Joint Distribution Committee and from private sources. In its decisions the Emergency Committee has had the support of the Rockefeller Foundation, which, in accordance with its general policy, has reserved freedom of action in regard to each application from the universities. Actually the Rockefeller Foundation has contributed equally with the Emergency Committee in almost all the grants made.

As of January 1, 1934, grants have been made for placing thirty-six scholars. Their names, disciplines, previous institutions and present posts are given below.

M. Palyi, economics, Handelshochschule, Berlin, University of Chicago.

O. Szasz, mathematics, Frankfurt, Massachusetts Institute of Technology.

[5] Lewis and Doody, *Jour. Am. Chem. Soc.*, 55: 3504, 1933.

[6] Pacsu, *Jour. Am. Chem. Soc.*, 55: 5056, 1933.

J. Franck, physics, Göttingen, Massachusetts Institute of Technology, the Johns Hopkins University.
K. Loewenstein, law, Munich, Yale University.
F. Bernstein, mathematics, Göttingen, Columbia University.
P. Tillich, theology, Frankfurt, Union Theological Seminary and Columbia University.
K. Lewin, psychology, Berlin, Cornell University.
O. Nathan, economics, Hochschule für Politik, Berlin, Princeton University.
K. Landauer, economics, Handelshochschule, Berlin, University of California.
H. Lewy, mathematics, Göttingen, Brown University.
E. Berl, chemistry, Technische Hochschule, Darmstadt, Carnegie Institute of Technology.
M. Sommerfeld, literature, Frankfurt, New York University.
H. Neisser, economics, Kiel, University of Pennsylvania.
E. Noether, mathematics, Göttingen, Bryn Mawr College.
Felix Bloch, physics, Leipzig, Stanford University.
R. Brauer, mathematics, Königsberg, University of Kentucky.
Moritz Geiger, philosophy, Göttingen, Vassar College.
Artur Nussbaum, law, Berlin, Columbia University.
K. Pribram, economics, Frankfurt, Brookings Institution.
Walter Beck, criminologist, Leipzig, Boston University.
H. Werner, psychology, Hamburg, University of Michigan.
Max Sulzbacher, biochemistry, Tierärztliche Hochschule, Berlin, Connecticut State College.

Definite selections have not yet been made by the Hebrew University, Palestine; the University of Wisconsin; the University of Minnesota; the University of Missouri; Duke University; the University of North Carolina; Rutgers University; Purdue University; the Catholic University of America; Mills College; the Ohio State University, and the University of Pennsylvania.

CHEMICAL ABSTRACTS

PROFESSOR E. J. CRANE, of the Ohio State University, has issued a statement describing the work of *Chemical Abstracts*, edited by him under the auspices of the American Chemical Society.

The volume for 1933 contains 64,190 abstracts, representing new information of chemical interest appearing in scientific journals throughout the world as well as reviews of the chemical patents granted in the various nations. This is a gain of 6,109 over 1932. Professor Crane points out that an increasingly large number of patents are chemical, not mechanical. Those issued in the principal countries of the world during 1933 aggregated 28,051, a record figure.

Chemical Abstracts was founded more than twenty-five years ago to keep American science and industry informed of chemical progress in other lands. The annual index number is so large that it has to be issued in three separate sections, each comprising about 800 pages. The 2,000 scientific journals which are systematically examined for articles of chemical interest by more than 200 abstractors yielded material for 36,139 abstracts in 1933 as against 37,403 in 1932. This substantial increase in total number of abstracts has been due to patent activity.

Most of these patents relate to chemical processes, as electroplating, metallurgy, fermentation, gas manufacture, petroleum refining, the making of acids, salts, dyes, paper, cement, pharmaceutical chemicals, explosives, pigments, sugar, etc., or to so-called compositions of matter as the many plastic and other artificial materials which the chemist has provided in recent years in such useful form that they are replacing natural materials long in use.

That investigative or at least publication activity has shown little effect of the depression is evident from the following figures:

Year	Abstracts of articles	Abstracts of patents	Total number of abstracts
1929	29,082	17,867	46,949
1930	32,731	21,246	53,977
1931	32,278	18,904	51,182
1932	37,403	20,678	58,081
1933	36,139	28,051	64,190

It is pointed out that these figures do not mean that chemical industry has escaped the depression, but they suggest that there has been continued active building for the future of chemistry.

THE AMERICAN ACADEMY OF TROPICAL MEDICINE

AT a Conference on Tropical Medicine held on February 5 and 6, under the auspices of the National Research Council in Washington, the new Academy of Tropical Medicine was formed and incorporated under the laws of the District of Columbia. Delegates to the conference included: Dr. George C. Shattuck and Dr. Richard P. Strong, Harvard Medical School; Dr. Francis W. O'Connor and Dr. James W. Jobling, College of Physicians and Surgeons, Columbia University; Dr. Charles F. Craig, Tulane School of Medicine; Dr. Henry E. Meleney, School of Medicine, Vanderbilt University; Dr. Robert Hegner and Dr. W. W. Cort, School of Hygiene and Public Health, the Johns Hopkins University; Dr. Edward B. Vedder and Dean Earl B. McKinley, School of Medicine, George Washington University; Dr. William H. Taliaferro, University of Chicago; Dr. Alfred C. Reed, Pacific Institute of Tropical Medicine, University of California; Dr.

Thomas T. Mackie, Cornell University Medical School; Dr. Howard T. Karsner, School of Medicine, Western Reserve University; Dr. Henry B. Ward, University of Illinois; Dr. Stanhope Bayne-Jones, School of Medicine, Yale University; Dr. Malcolm H. Soule, University of Michigan Medical School; Dr. Mark F. Boyd, International Health Division, Rockefeller Foundation; Dr. Theobald Smith, representing the Rockefeller Institute of New York and Princeton; Mr. Perry Burgess, president, Leonard Wood Memorial for the Eradication of Leprosy; Dr. George W. Bachman, School of Tropical Medicine, San Juan, Puerto Rico; Dr. Robert Hegner, Gorgas Memorial; Dr. Henry Hanson, State Board of Health of Florida; Dr. R. C. Connor, Medical Department, United Fruit Company; Major James S. Simmons, Medical Corps, United States Army; Lieut. Commander Sterling S. Cook, Medical Department, United States Navy; Dr. L. R. Thompson, United States Health Service; Dr. Bolivar J. Lloyd, Pan American Sanitary Bureau, and Maurice C. Hall, Bureau of Animal Industry, Department of Agriculture.

Other guests at the conference included Dr. Isaiah Bowman, chairman of the National Research Council, and Dr. Francis G. Blake, chairman of the Division of Medical Sciences of the National Research Council, both of whom gave addresses of welcome to the conference delegates. Others who attended the various meetings and functions were Surgeon General Robert U. Patterson, U. S. Army Medical Corps; Surgeon General P. Rossiter, Medical Department, United States Navy; Surgeon General Hugh S. Cumming, United States Health Service; Dr. John R. Mohler, director, Bureau of Animal Industry; Captain Wm. H. Bell, director of the United States Naval Medical School; Colonel P. W. Huntington, director of the United States Army Medical School; Dr. George W. McCoy, director of the National Institute of Health, and Dr. William Charles White, medical director of the Committee on Research, National Tuberculosis Association.

The Conference on Tropical Medicine which led to the formation of the new Academy of Tropical Medicine was an outgrowth of the work of a Committee on Survey of Tropical Diseases, in the National Research Council, of which Dr. E. B. McKinley is director of studies and which began its world survey early in 1933. Dr. Frederick P. Gay, chairman of this committee, presided, and Dr. Richard P. Strong, also a member of the committee, presided at the dinner which was held at the Mayflower Hotel on the evening of February 5.

At the final session of the academy the following officers were elected: *President,* Dr. Theobald Smith; *Vice-president,* Dr. Charles F. Craig; *Treasurer,* Professor W. W. Cort; *Secretary,* Dr. Earl B. McKinley; *Members of the Council,* Dr. Stanhope Bayne-Jones for five years, Dr. Herbert C. Clark for four years, Dr. Richard P. Strong for three years, Dr. Alfred C. Reed for two years and Dr. Henry E. Meleney for one year.

Among the purposes for which the Academy of Tropical Medicine is formed are the furtherance and extension of knowledge for the prevention of human and animal diseases of warm climates by stimulating interest, inquiry and research into their distribution, causes, nature, treatment and methods of control; through designated committees in the several fields of knowledge contributing to tropical medicine, to provide a current survey of work in progress in tropical medicine and sanitary and hygienic work related thereto; to coordinate American work in tropical medicine to the end that unnecessary duplication and overlapping shall be avoided as far as possible, and that valid lines of study shall not be neglected; to function as a central source of information for the advantage of investigators in this field of knowledge; to cooperate with other agencies interested in maintaining and obtaining support for tropical medicine, both in a financial way and to the end that the medical professions, the general body of scientific workers and the general public may be better informed regarding the values and needs of tropical medicine in national and international programs; and to receive funds and administer them through grants-in-aid and in support of definite projects related to the purposes of the academy.

At the close of the conference a gold medal was provided to the academy through the courtesy of President Cloyd Heck Marvin, George Washington University, to be awarded for distinguished work in the field of tropical medicine by the academy at appropriate intervals. President Marvin in tendering the medal to the academy did so in memory of the tradition of such members of former faculties in the School of Medicine in George Washington University as Walter Reed, Theobald Smith, Frederick F. Russell and Charles F. Craig—all names of prominence in the field of American tropical medicine. The names of charter members of the academy, not to exceed fifty in number, are to be announced within three months from the date of the conference.

EARL B. MCKINLEY, M.D.,
Secretary

RECENT DEATHS

DR. WILLIAM MORRIS DAVIS, since 1912 emeritus professor of geology at Harvard University and during the last three years professor of physiographic geology at the California Institute of Technology, died on February 5 in his eighty-fourth year.

DR. EDWARD W. WASHBURN, since 1926 chief chem-

ist of the U. S. Bureau of Standards, died suddenly on February 6. He was sixty-one years of age.

JOHN HAROLD MORECROFT, professor of electrical engineering in Columbia University, died on January 26, at the age of fifty-two years.

ROBERT HENRY WOLCOTT, chairman of the department of zoology, University of Nebraska, died on January 23, at the age of sixty-five years.

DR. EDGAR L. TAGUE, professor of chemistry and assistant in protein chemistry for the Agricultural Experiment Station of the Kansas State College of Agriculture and Applied Science since 1914, has died.

DR. PAUL L. SAUREL, formerly head of the department of mathematics of the College of the City of New York, died suddenly on January 21. He was sixty-three years old.

THE death is announced at the age of sixty-four years of Andrew C. Life, professor of botany and member of the faculty of the University of Southern California since 1907.

DR. DUKINFELD HENRY SCOTT, British paleobotanist, formerly professor of botany at the Royal College of Science, died on January 29, at the age of seventy-nine years.

SIR WILLIAM BATE HARDY, director of food investigation of the Department of Scientific and Industrial Research, died on January 23, at the age of sixty-nine years.

DOUGLAS WILLIAM FRESHFIELD, known for his work in geographical exploration, formerly president of the Royal Geographical Society, died on February 9, in his eighty-ninth year.

SCIENTIFIC NOTES AND NEWS

PROFESSOR HAROLD C. UREY, of Columbia University, has been awarded the Willard Gibbs Medal of the Chicago Section of the American Chemical Society for his discovery of "heavy water." The medal is awarded annually by a national jury to one "whose work in either pure or applied chemistry has received world-wide recognition." The jury for 1934 was composed of Dr. Lee F. Supple, Lewis Institute, Chicago, chairman; Professor Joel H. Hildebrand, University of California; Professor Charles A. Kraus, Brown University; Dr. Carl S. Miner, Miner Laboratories, Chicago; Professor Julius Stieglitz, University of Chicago; Professor Roger Adams, University of Illinois; Dr. Harrison E. Howe, editor of *Industrial and Engineering Chemistry;* Dr. Phoebus A. Levene, Rockefeller Institute; Professor Hermann I. Schlesinger, University of Chicago; Professor Edward C. Franklin, Stanford University; Professor Moses Gomberg, University of Michigan; Professor Ross A. Gortner, University of Minnesota; Dr. Willis R. Whitney, General Electric Company.

DR. J. B. WHITEHEAD, dean of the School of Engineering of the Johns Hopkins University and president of the American Institute of Electrical Engineers, has been made an honorary member of the French Society of Electricians. On the occasion of the celebration of its fiftieth anniversary last month the society awarded this distinction to a leading electrical engineer in each of the larger European countries, in Japan and in the United States.

PROFESSOR FRANCIS CARTER WOOD, director of the Institute of Cancer Research, Columbia University, has been made an honorary member of the Norwegian Society for Medical Radiology.

DR. VICTOR MORITZ GOLDSCHMIDT, professor of mineralogy at Göttingen, has been elected an honorary member of the Mineralogical Society of Great Britain.

DR. FRANCESCO SEVERI, professor of mathematics at Rome, has been elected a corresponding member of the Prussian Academy of Sciences.

A GROUP of fifty friends and former students of Professor Albert Sauveur, Gordon McKay professor of metallurgy and metallography in the Graduate School of Engineering of Harvard University, recently gave a dinner in honor of his seventieth birthday. Many congratulatory letters and telegrams were received from distinguished metallurgists both in this country and abroad.

DR. JOHN ELMER WEEKS, professor emeritus of ophthalmology, New York University and Bellevue Hospital Medical School, was the guest of a group of medical friends at a dinner recently given in honor of his eightieth birthday. Dr. Frederick A. Kiehle presided and Drs. Hugh Cabot, Rochester, Minnesota; Ralph F. Davis and Ralph A. Fenton made addresses.

IN expression of appreciation of the work of Sir Flinders Petrie, on the occasion of his retirement from the Edwards professorship of Egyptology at University College, London, it is planned to present a portrait to University College. An appeal for funds for this purpose has been issued over the names of Professor J. H. Breasted, M. J. Capart, Dr. Howard Carter, Professor F. Ll. Griffith, Sir George Hill, Sir Henry Lyons, Dr. Allen Mawer, Sir Robert Mond and Dr. Margaret Murray. Subscriptions towards the fund will be received by Sir Henry Lyons, F.R.S., 3 York Terrace, Regent's Park, London, N.W. 1.

THE Edward L. Bok Award of $10,000 given annually to the "first citizen" of Philadelphia was presented on February 8 to Dr. Lucy L. W. Wilson, principal of the South Philadelphia Girls' High School.

THE secretary of the London Geological Society announces that the council has this year made the following awards: The Wollaston Medal to Sir Henry Alexander Miers, honorary professor of crystallography in the University of Manchester, for his researches on the mineral structure of the earth; the Murchison Medal to Dr. George Hickling, professor of geology in Armstrong College, Newcastle-on-Tyne, for his contributions to geology, especially in the stratigraphy of the coal measures and the structure of coal; the Lyell Medal to Dr. Finlay Lorimer Kitchin, Geological Survey, in recognition of his contributions to paleontology (Dr. Kitchin died on January 20); another Lyell Medal to the Rev. Walter Howchin, of the University of Adelaide, for his geological and paleontological researches in Australia; the Wollaston Fund to Dr. William Richard Jones, of the Royal School of Mines, for work in economic geology and recent investigations on silicosis; the Murchison Fund to Dr. John Wilfrid Jackson, assistant keeper in the Manchester Museum, for his contributions to Pleistocene geology and paleontology and to malacology, and the Lyell Fund to Mr. Frederick William Shotton, in recognition of his work on the upper Paleozoic and Quaternary rocks of the Midlands.

THE Central Executive Committee of the Russian Soviet Republic has awarded the title "Honorable Science Workers" to Professors M. J. Averbach and A. A. Kisel, of the Second Moscow Medical Institute, and to Professor V. P. Osipov, director of the Institute for the Study of the Brain. Professor Averbach founded a school of ophthalmology, organized a large hospital for eye diseases and is the permanent president of the Moscow Ophthalmologic Society. Professor Alexander A. Kisel, who organized the children's clinic of the Second Moscow Medical Institute, where he worked about twenty years, is the permanent president of the All-Union and Moscow district societies of pediatricians. Professor Victor P. Osipov since 1920 has been the director of the State Institute for the Study of the Brain at Leningrad.

OFFICERS of the History of Science Society elected for 1934 are as follows: *President,* Dr. Harvey Cushing, School of Medicine, Yale University; *First Vice-president,* Dr. Charles A. Browne, U. S. Bureau of Chemistry and Soils; *Second Vice-president,* Dr. Chauncey D. Leake, Medical School, University of California; *Chairman of the Publications Committee,* Dr. George S. Brett, University of Toronto; *Corresponding Secretary and Treasurer,* Frederick E. Brasch, Library of Congress; *Recording Secretary,* Dr. Lao G. Simons, Hunter College, New York City. *New members of the Council,* Dr. Frederick Barry, Columbia University; Dr. Richard H. Shryock, Duke University; Dr. Dorothea Waley Singer, London; Dr. Louis C. Karpinski, University of Michigan; Dr. Charles A. Morris, University of Chicago, and Dr. Raymond C. Archibald, Brown University.

DR. PERCY E. RAYMOND, professor of paleontology at Harvard University and curator of invertebrate paleontology in the Museum of Comparative Zoology, has been elected president of the Paleontological Society of America and a vice-president and member of the council of the Geological Society of America.

DR. W. A. F. BALFOUR-BROWNE, until his retirement in 1930 professor of entomology in the Imperial College of Science and Technology, London, was recently elected president of the Royal Microscopical Society.

DR. KIRTLEY F. MATHER, professor of geology at Harvard University, has been appointed director of the Harvard Summer School for 1934. Dr. Mather succeeds Assistant Professor Henry N. Black.

CEDRIC H. GUISE, assistant professor of forest management and utilization at the New York State College of Agriculture at Cornell University, has been promoted to a professorship.

DR. G. P. WRIGHT has been appointed to the Sir William Dunn chair of pathology tenable at Guy's Hospital Medical School. Since 1931 he has been assistant lecturer in morbid anatomy and curator of the Museum at University College Hospital Medical School and also pathologist to the hospital.

A NEW chair for racial hygiene has been founded in Berlin, with Professor Fritz Lenz, of Munich, as its first occupant. Professor Lenz has also been appointed departmental director for racial hygiene and eugenics at the Kaiser Wilhelm Institute for Anthropology.

DR. E. S. PEARSON has been appointed reader in statistics in the University of London.

MAJOR LAWRENCE H. DUNN, who has been medical entomologist and assistant director of the Gorgas Memorial Laboratory at Panama, during the past five years, has resigned and will soon return to the United States.

DR. HAVEN EMERSON, formerly health commissioner of New York City, now a professor of preventive medicine and public health in Columbia University, and Dr. Frank L. Babbott, Jr., have been appointed as members of the Board of Health, replacing Dr. Harry P. Swift and Dr. R. Percy Crandall.

PROFESSOR GREGORY P. BAXTER, Theodore William Richards professor of chemistry at Harvard Univer-

sity, has been appointed a member of a committee to advise on the use of the money contributed by the Rockefeller Foundation and by others for geophysical research.

FRED J. SIEVERS, director of the Graduate School and of the Experiment Station of the Massachusetts State College at Amherst, has been appointed state supervisor of a farm finance survey for Massachusetts.

THE *British Medical Journal* reports that Professor W. W. Jameson, dean of the London School of Hygiene and Tropical Medicine, arrived in Colombo on January 9. He is touring Ceylon and India with representatives of the Rockefeller Foundation. In view of the amalgamation of the Ross Institute with the London School of Hygiene and Tropical Medicine, Professor Jameson will, before returning, visit some of the Ross Institute research centers in Assam and Bengal, and will afterwards proceed to Malaya. During his absence, Professor R. T. Leiper has been appointed acting dean of the London School. Professor Jameson is expected back in London in April. Professor J. Gordon Thomson, director of the department of protozoology, is leaving London on January 31, and will travel by air to South Africa. He will proceed to East Africa, where he will spend six months in research work on malaria and sleeping sickness, with special reference to immunity.

AT the two hundred and fifty-sixth meeting of the Washington Academy of Sciences on January 18, Dr. Robert F. Griggs, professor of botany at the George Washington University, delivered his address as retiring president on "The Problems of Arctic Vegetation."

DR. WILLIAM L. BRAGG, professor of physics at Manchester University, arrived in New York City on February 5 on his way to Ithaca, where he is lecturing at Cornell University, under the auspices of the Baker Foundation.

DR. ERMINE C. CASE, professor of historical geology and paleontology of the University of Michigan, director of the Museum of Paleontology and curator of vertebrates for the University Museum, will be the ninth Henry Russell lecturer at the university.

DR. HARLOW SHAPLEY, director of the Harvard College Observatory, delivered the third annual James Arthur Lecture on "Time and its Mysteries" on February 6 at New York University. The James Arthur Foundation was established in 1931 with a bequest from the late James Arthur, New Rochelle manufacturer and collector. In addition to providing an annual lecture on "Time," the foundation maintains and enlarges the James Arthur Collection of Timepieces at New York University. The collection, valued at more than $150,000, is one of the largest and most comprehensive historical groups of clocks and watches in the world.

DR. ERNEST W. BROWN, emeritus professor of mathematics at Yale University, read a paper on "Time and its Determination" before the American Philosophical Society, Philadelphia, on February 2.

DR. HARLAN T. STETSON, director of the Perkins Observatory of the Ohio Wesleyan University, gave an illustrated lecture before the Rittenhouse Astronomical Society at Philadelphia on February 13.

DR. LOUIS T. MORE, dean of the Graduate School of the University of Cincinnati, on February 2 addressed the Ohio State University Chapter of the Society of Sigma Xi on "What Constitutes a Law in Science."

DR. ETIENNE B. RENAUD, professor of anthropology at the University of Denver, was the speaker for the January meeting of the Sigma Xi Club. He gave an illustrated lecture on "Old and New World Cultures."

PROFESSOR R. H. FOWLER gave a lecture on February 2 on "Heavy Hydrogen" under the auspices of the Liversidge Foundation of the University of Cambridge.

THE Federation of American Societies for Experimental Biology, formed by the American Physiological Society, the American Society of Biological Chemists, the American Society for Pharmacology and Experimental Therapeutics and the American Society for Experimental Pathology, will meet in New York City on March 28, 29, 30 and 31, under the auspices of the College of Physicians and Surgeons, Columbia University. The Hotel Pennsylvania will serve as headquarters of the federation and all scientific sessions, except the demonstrations of Friday afternoon, March 30, will be held in the hotel. These will be given in the laboratories of the College of Physicians and Surgeons, Columbia University.

AT its December meeting the American Physical Society formally ratified the formation of a Metropolitan Section of the society. There are more than 400 members of the society in or near New York and it is proposed to bring these together occasionally to aid the exchange of ideas and advance the science of physics. An informal organizing meeting was held on October 27 at Columbia University. On this occasion a constitution was adopted, officers elected and the meeting then listened to invited papers by Professor Rudolph Ladenburg, of Princeton University; Professor George B. Pegram, J. R. Dunning and Professor Harold C. Urey, of Columbia University. The section will probably meet again near the end of March and thereafter several times a year. The officers at present are:

Chairman, George B. Pegram; *Vice-chairman,* W. S. Gorton; *Secretary-Treasurer,* Henry A. Barton; *Members of the Executive Committee,* I. I. Rabi and G. Breit. Membership in the section is restricted to members of the American Physical Society, but its meetings will be open to all interested persons.

THE scientific library of the late Professor U. S. Grant, containing fifteen hundred bound and four thousand unbound volumes, was formally presented by Mrs. Grant on February 14 to the Department of Geology and Geography of Northwestern University. President Scott accepted the library on behalf of the university. Dr. Grant was head of the department for the thirty-three years preceding his death in September, 1932.

DISCUSSION

THE BIOCHEMISTRY OF ANESTHESIA

DUE to travel and other external circumstances, the publications of W. D. Bancroft, *et al.,* printed in the *Journal of Physical Chemistry* (35: 215, 1931, and 36: 273, 1932) were unfortunately traced only during the course of the autumn of this year by means of the *Chemisches Zentralblatt.* The reprints of those papers sent to me upon request reached me at the beginning of October, 1933, and so I may be permitted to refer to them briefly after an undue delay.

(1) The assumption referred to on page 216—that "it is known that during narcosis the permeability is first lowered and then increased"—has been abandoned by Hoefler and Weber in 1926,[1] and Nord and Franke expressed their position concerning this point as well as concerning the alleged "stimulation" or "activation" by means of ethylene in their extensive experiments with zymase solutions and yeast cells[2] as follows: "The hitherto unexplained effect of minute quantities of ethylene and related substances on cell systems appears to be due to an *initial* increased cell permeability, allowing an intensified interaction between reactants and enzymes, followed by the formation of a (reversible) adsorption film, which simultaneously acts as a protector against damaging transformation products."

(2) Nitrous oxide and acetylene does not belong to the same group of narcotizing agents, since, according to another series of investigations,[3] the former decreases the surface tension, whereas the latter (as well as ethylene) increases the surface tension of solutions of biocolloids. The "Erstickungstheorie" of Herm. Wieland can not be, therefore, valid, and on account of the opposed working mechanism of the two gases the adsorption theory can also not be regarded as satisfactory. Besides this, we could show by nephelometric measurements that solutions of biocolloids are not coagulated, either by nitrous oxide or by unsaturated hydrocarbons, and so we could not confirm any connection between narcosis and coagulation, all the less, since the activity of zymase solutions could be practically inhibited by the latter[4] without a noticeable coagulation of the carriers.

Besides many other statements in the papers of Bancroft *et al.,* which stimulate the reader to constant mental discussion, I wanted to refer especially to the above proven contradictions.[5]

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TIERÄRZTLICHE HOCHSCHULE
BERLIN, NOVEMBER 25, 1933

NAMING HYDROGEN ISOTOPES

THE wide-spread interest in heavy hydrogen and its compounds has been reflected in the discussion of suitable names and symbols for both H^1 and H^2. Of the letters in SCIENCE one of the most interesting is that of Professor Urey and others in the number dated December 29.

The awkwardness of the names protium and deuterium, however suitable they may be scientifically, appears to be commonly recognized. Various alternatives have been offered, but I have failed to see that any suggestion has been made of the following rather simple method of meeting the requirements for both names and symbols for these isotopes.

Our minds, as well as our literature, are so filled with the specific significance of the name hydrogen that to discard it would be certain to entail endless confusion. Both simplicity and understanding would be served by calling protium "hydrogen-p" and deuterium, "hydrogen-d," and the connection with the familiar hydrogen thus be maintained. Similarly, the symbols Hp and Hd would be specific, exact and almost self-explanatory.

The formulas H^1H^2, $NH^1H^2_2$, $NH^1_2H^2$ and $C_6H^1_2H^2_4$, cited by Professor Urey and others, would then be written HpHd, $NHpHd_2$, NHp_2Hd and $C_6Hp_2Hd_4$, thus reducing the symbols to a form

[1] *Jahrbuch f. wiss. Botanik,* 65: 643–737.

[2] *Protoplasma,* 4: 595, 1928; *Jour. of Biolog. Chemistry,* 79: 50, 1928; *Z. f. angewandte Chemie,* 42: 1025, 1929. "Mechanism of Enzyme Action and Associated Cell Phenomena," Baltimore, Md., 1929.

[3] *Trans. Faraday Society,* 26: 760; *Z. f. Physikal. Ch.* (A) 150: 1, 1930, and 166: 1, 1933, and the monograph, "Zum Mechanismus der Enzymwirkung unter besonderer Berücksichtigung der Kryolyse," Stuttgart, 1933.

[4] *Z. f. Physiolog. Ch.,* 183: 217, 1929.

[5] Compare for further literature: "Ergebnisse der Enzymforschung," Vols. 1 and 2, Leipzig, 1932, 1933.

analogous to those in common usage. The names would be, most simply, hydrogen-pd; ammonia-pd_2, ammonia-p_2d and benzene-p_2d_4. Professor Whitmore's deuteroneopentane would become neopentane-d.

It is unlikely that this nomenclature will meet all complications, but for the simpler compounds it appears to have advantages and may indeed suggest to others a more perfect solution of the problem.

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THE NEUTRON

THE experiments of Lawrence, Livingston and Henderson on the artificial disintegration of the deuton by the proton show that the mass of the neutron as calculated from these experiments is much smaller than the value assigned to it by Chadwick. If we express this mass difference in energy units, by means of the Einstein relation, then this difference is equivalent to 6 million volts. This difference is much greater than the mean error involved in the two sets of experiments. This indicates either that the relationships used in the calculation of the mass are not valid or that the neutron may have a different mass, depending on the conditions where it exists.

In the light of these experiments on the mass of the neutron and others on the constituents of the beryllium nucleus, our ideas of the stability of the nucleus must be revised. We meet here a similar situation to that which arose in classical electrodynamics when it attempted to account for the stability of the atom. The stability arises not from the binding energy of the particles for one another but from the existence of quantum laws governing the system. If we suppose that the neutrons are held in the nucleus by a type of quantum law and that the binding, if any, plays no rôle, then we can apply Dirac's theory of radiation to the behavior of the neutron, substituting in that theory for the energy and momentum of the photon the appropriate quantities for the neutron. Instead of the atom in various quantum states of excitation we have the nucleus in its various quantum states forming stable configurations—the positron plus the neutron, the proton; the positron plus two neutrons, the deutron and other combinations of positrons and neutrons. The interaction between the positron and the neutron which takes the place in the neutron theory of the interaction between the light wave and the electron is not known as yet. However, experiments on the scattering of neutrons should give us some insight into the type of interaction.

The experiments on the production of neutrons by alpha particles gives neutrons, in many cases, of widely different energies. These sets of neutrons of different energies have been assumed generally to be made possible by the emission of gamma rays from the nucleus, but in the case of beryllium it is difficult to see how any nucleus could emit rays of such great energy. Perhaps, the difference may arise from the production of the neutron occurring by different reactions. For example, in the case of beryllium, we might have as the final products of the disintegration either a neutron and a carbon nucleus or a neutron and three alpha particles.

ARTHUR BRAMLEY

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OXYGEN AS AN ACCELERATOR IN THE GROWTH OF EMPUSA ON FLIES

WHILE occupied with certain physiological experiments with house flies some time ago the writer accidentally left several flies in an atmosphere of O_2 for a period of several weeks. It was observed at the end of this time that the flies were densely covered with *Empusa*. The growth of the fungus was much more luxuriant than the writer had ever observed before. It seemed probable that the O_2 atmosphere had stimulated the development of the *Empusa*. Consequently, experiments were conducted with more flies to investigate the problem further.

In almost every trial, in which freshly killed or live flies were placed in glass jars with glass stop-cocks and the air replaced with O_2, the fungus developed. Some of the flies were so covered with the growth that scarcely any body parts were visible. They gave the appearance of cottony balls.

House flies captured in the spring and placed in O_2 atmospheres did not develop *Empusa*. The experiments were again repeated the following fall with success. It was noted, however, that jars containing the proper moisture developed the best *Empusa* growths. No attempts were made to determine the correct humidity, as the writer was interested only in the O_2 effects. However, it is safe to conclude that O_2 atmospheres greatly accelerate the development of *Empusa* on flies.

Ordinarily a period of one to two weeks is necessary for the *Empusa* to develop in jars of O_2. Calcium chloride tubes can be used in place of jars with glass stop-cocks; in fact any sort of glass container that can be hermetically sealed should be satisfactory.

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THE STIMULATIVE ACTION OF YEAST EXTRACT IN THE RESPIRATION OF RHIZOBIUM

DURING the past two years respiration experiments with the legume bacteria have been conducted in this laboratory with the Warburg technique, special con-

sideration being given to the nitrogen requirements of the organisms. Inasmuch as it was well known that yeast extract stimulated growth of the organisms, this material was included in the studies along with various organic and inorganic compounds of nitrogen, the yeast extract being used in amounts proportional to its nitrogen content. In all the experiments conducted the yeast extract was found to stimulate oxygen consumption, and therefore respiration, to a considerably greater extent than any of the nitrogen compounds used. The extent of respiration was somewhat proportional to the amount of yeast extract in the medium.

In reporting these investigations at the thirty-fourth annual meeting of the Society of American Bacteriologists at Ann Arbor, in December, 1932, and more completely in papers that are now in the process of publication, the conclusion was drawn that the stimulative effect of the yeast extract may have been due, aside from the nitrogen it contained, to other factors which would serve to stimulate oxygen consumption. While no reference has been made in these reports to a respiration co-enzyme in the yeast extract, the suggestion has been offered that the stimulation may have been caused by vitamins, auximones or other similar substances.

In view of these results, the author was unusually interested in the recent report in SCIENCE by Allison, Hoover and Burk,[1] who reported to have found a specific factor which is essential for respiration. In studies with the root nodule bacteria of leguminous plants these investigators found that respiration increased from a small value in the presence of a trace of the factor, to as high as 1,000 cmm O_2 per mg of dry weight per hour at 31° C. in its presence. This factor, considered by them as a respiration co-enzyme, was secured by extracting commercial sucrose with absolute alcohol. It was also claimed that, aside from furnishing readily available nitrogen, the chief rôle of the yeast water in culturing the nodule bacteria is to supply a source of the essential respiration factor.

The conclusions concerning the stimulative action of yeast extract on the respiration of the root nodule bacteria are in agreement with and confirmatory to the results secured in this laboratory. Our work has not as yet been directed toward the characterization and isolation of the stimulative factor, and it has, therefore, not shown the presence in yeast extract of a "specific factor essential to respiration." But the preponderance of evidence points to the conclusion that the beneficial effects of the yeast extract lies chiefly in its content of readily available supply of nitrogen and energy material.

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SOCIETIES AND MEETINGS

INTERNATIONAL GEOGRAPHICAL CONGRESS OF 1934

THE International Geographical Congress of 1934 will be held in Warsaw, Poland, from August 23 to 31, inclusive, under the presidency of Dr. Isaiah Bowman. Organization of the congress is in the hands of a Polish executive committee, of which Professor Eugene Romer is chairman and Professor Stanislas Pawlowski general secretary. The address of the congress and its secretariat is High School of Commerce (Szkola Glowna Handlowa), 6 Rakowiecka Str., Warsaw, Poland.

At a time when financial depression and unfavorable exchange rates may cause some to hesitate about planning participation in the congress, it seems appropriate to review the great advantages to be gained by such participation. Americans who attended the recent congresses in Cambridge (1928) and Paris (1931) and who took part in the interesting and instructive excursions arranged by the local organizing committees found the experience highly profitable.

One of the principal objects of the congress is to bring together, from all parts of the world, the outstanding leaders in all phases of geography. The opportunity to become personally acquainted with these men, to learn about their most recent investigations and to make them acquainted with one's own work is to be highly prized.

At the Cambridge meeting official delegates from thirty-one countries were present. At Paris forty-six countries were represented. Experience shows that in these different countries, with their differing opportunities for pursuing investigations of various phases of geography, there frequently develop marked differences in methods of study and in nature of results secured. The consequent interchange of ideas effected at the congresses is in the highest degree stimulating.

America stands for certain types of work in geography which have been developed in this country to an extent not observed elsewhere. The prestige enjoyed by our work abroad, as well as its progress at home, will be enhanced by its effective representation at the congress. An obligation thus rests upon American geographers to do all in their power to assure effective representation of their science at Warsaw. In view of the fact that American geography is to be honored at the congress by having one

[1] SCIENCE, 78: 217–218, 1933.

of its leaders as president of the international gathering, it seems peculiarly appropriate that American geographers should make a special effort to send to Warsaw a large and representative group.

As a compliment to Professor Eugene Romer and in recognition of his long and distinguished service in the field of Polish cartography, it has been suggested that the American delegation might devote special attention to preparing an effective cartographic exhibit at Warsaw. Steps toward achieving this end are being taken.

Attention is directed to the fact that the meeting of the congress in Warsaw not only offers an unusual opportunity for American geographers to study, under exceptionally favorable conditions as regards both expense and skilled leadership, both the physical and the human geography of Poland on the excursions that precede and follow the congress, but also provides the occasion for visiting other parts of central and eastern Europe and the Near East.

Any person engaged in scientific research in the field of geography or interested in the results of geographical research may enroll as member of the congress. Moreover, representatives of government institutions, scientific societies, universities and other educational institutions, in countries belonging to the International Geographical Union (this includes the United States), may take part in the congress; these delegates should duly enroll as members of the congress. Persons not geographers but who have geographical interests, or who represent institutions not primarily geographic but which have large geographical interests (such as federal and state geological surveys, departments of agriculture, museums of natural history, etc.), are welcome. If such individuals plan for other reasons to be in Europe in the summer of 1934, they should arrange if possible to participate in the congress. Persons belonging to the families of members of the congress may take part in the congress, if they duly apply for membership to the secretariat.

Admission as a member of the congress will be granted upon filling in the form of application for membership and upon payment of the fee of 40 zlotys (approximately $7.50 at current exchange) and 10 zlotys for each person accompanying the regular member. Application blanks may be secured from the Secretary of the National Committee of the United States, Mr. W. L. G. Joerg, American Geographical Society, Broadway at 156th Street, New York City.

It is earnestly recommended that American geographers, whether or not they are able to go to Warsaw, should enroll themselves promptly as members of the congress. It is understood that the postal authorities may decline to issue a money order in zlotys, in which case prospective members should ascertain, at post office or bank, the rate of exchange on the date the money order is to be secured and get a money order for the equivalent, payable in American dollars. The value of the moderate enrolment fee will be returned in double measure in the form of publications of the congress. At the same time valuable support will be given to those charged with organizing the congress and a representative American membership will be assured.

The United States Government has taken official cognizance of the International Geographical Congresses, and through the Secretary of State has arranged for representation of the Government by a limited number of delegates. It is requested that those planning to be present at Warsaw notify the secretary, Mr. W. L. G. Joerg, in order that suggestions for appointment as delegates may in due course be made from among the number who will attend. Attendance of American delegates is always at their own expense.

Members may be appointed to represent public and scientific institutions, including universities. In such case no formality is necessary except notification by the institution to the secretariat of the congress.

Those planning to attend the congress will find further data of interest to them in the circulars of information issued by the committee on organization. Copies may be secured by addressing Mr. W. L. G. Joerg, American Geographical Society, Broadway at 156th Street, New York City.

DOUGLAS JOHNSON, *Chairman,*
CURTIS F. MARBUT, *Vice-chairman,*
National Committee of the United States
W. L. G. JOERG, *Secretary,*
International Geographical Union.

SCIENTIFIC APPARATUS AND LABORATORY METHODS

THE DEMONSTRATION OF INTACT MAMMALIAN AND AMPHIBIAN NERVOUS SYSTEMS BY MACERATION OF WHOLE ANIMALS

THE demonstration of intact nervous systems of common laboratory animals has proved to be a useful device in the teaching of undergraduate biology. The method of producing such an effect is simple.

In the case of the frog, simple immersion in a solution of 30 per cent. nitric acid for a period of 24 hours results in complete decalcification of all bone, the disappearance of all connective tissue and more

or less complete maceration of the musculature of the animal. The nervous system, at least the brain, the spinal cord, the spinal nerves and the limb plexuses, remains intact. The action of nitric acid seems to be sequential in effect, acting first on the bone, then on the connective tissue. The result is the dissolution of the skeletal and muscular organization. The epithelium all over the body, both externally and internally, is soon disintegrated and the viscera do not long retain their organization.

The sympathetic nervous system does not seem to endure the maceration process. On the other hand, the central nervous system, together with the spinal nerves, resists the action of nitric acid for some time, with the result that by simple and careful teasing of the macerated tissue by pointed glass rods the whole nervous system is obtained complete or nearly so, depending on the care used in teasing. Most of the spinal nerves and the central nervous system are composed of relatively heavy myelinated fibers, while the fibers of the sympathetic nervous system are not or only very thinly so. It may well be that the presence of myelin sheaths slows up the action of nitric acid on the spinal nerves and the central nervous system for a long enough time to allow complete maceration of the rest of the body.

In the case of mammals, such as rats and guinea pigs, somewhat more care and apparatus are required for the best results. As the first action of nitric acid is the decalcification of bone, some care in extracting of the brain must be exercised. If the whole animal is simply immersed, as in the case of the frog, the head, consisting chiefly of the bony skull, is soon disintegrated. Thus the brain is ready long before sufficient maceration of the rest of the body has taken place. The precaution is necessary because the action on the nervous system appears to be one of hardening, the result being that if the immersion of some areas, particularly the brain, is too long, say 24 hours or longer, such tissue becomes exceedingly brittle, making easy handling of it practically impossible.

Maceration of complete small laboratory animals, such as rats or guinea pigs, should be carried out somewhat as follows: (1) Skin the animal completely, including the tail, the appendages and the head. This is to make the action of the acid on the bones and muscles more uniform and rapid. (2) Form a glass hook from glass rodding or tubing large enough to pass under the neck of the animal. Suspend the head by this hook from some suitable level above the immersion bath. (3) Immerse the rest of the mammal in the bath of 30 per cent. nitric acid for 36 hours. A large crystallizing dish or large Petri dish is perhaps the most satisfactory container for the bath. These dishes should not exceed 10 to 15 cm in depth. (4) After the 36-hour period allow the head to be immersed in the bath, leaving the head and body for another 8 hours. (5) Carefully tease the macerated muscle away from the nervous system. Not all the muscle tissue can be removed in this way. After all is removed that can be with safety, transfer the nervous system from the acid bath to an empty Petri dish of the same size as used for the bath. This transference can be easily done by simply inserting one of the glass rods under the brain and lifting the tissue out of the liquid. (6) Place the tissue in the empty Petri dish under a water faucet and let a stream of water gently drop on the tissue from some height. The force of the water will generally remove the remaining muscle tissue clinging to the nervous system.

The intact nervous system can be preserved in glycerine jelly, this method making a very satisfactory means of demonstrating to members of biology classes.

In such preparations by maceration, it will be found that the nervous system at the tips of the extremities, together with some of the spinal nerves, are occasionally lost. Skinning of the animals (unnecessary with frogs) aids in preventing this. In addition any loss can be considerably reduced if the most careful teasing is employed. The whole process seems to depend finally on the length of time of immersion. Inasmuch as the nervous system is quite resistant to the action of the acid, a period of 24 hours in the case of frogs and 48 hours with small laboratory mammals is not too long, excepting for the head region of mammals, immersion of which should not exceed 8 to 10 hours.

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THE ABSORPTION OF METHYLENE BLUE BY THE NEPHRIDIUM OF THE EARTHWORM

THAT the method for demonstrating the nephrostomes of the earthworm, published several years ago,[1] has met with some lack of success in the hands of certain workers has recently come to my attention. This difficulty is apparently due to failure to inject the dye in quantity sufficient to render the treated segments turgid. Under conditions of semi-flaccidity in the worm, the dye, when introduced dorsally, may fail to reach those parts of the nephridium lying near the mid-ventral axis.

In further work on the staining effect of methylene blue on the annelid nephridium, a better technique has been devised. An earthworm is anesthetized by immersion in a 0.2 per cent. aqueous solution of

[1] Elbert C. Cole, "The Demonstration of Nephrostomes in the Earthworm." SCIENCE, 62: 50–51, 1925.

chloretone, a mid-dorsal incision is made in the postclitellar portion of the body and the septa carefully cut away so that the specimen may be pinned out flat on the wax pad of a dissecting dish. The digestive tract is then removed, care being taken to cut the septa close to the under-surface of the intestine, in order that the nephridia may remain undisturbed. The preparation is then wet with several drops of methylene-blue saline solution. Methylene blue, lot NA3, certified by the Commission for the Standardization of Biological Stains, has proved entirely satisfactory for this purpose. For anatomical study one part of dye in 2,000 parts of 0.6 per cent. NaCl solution is a suitable concentration; for the study of cilia in motion a concentration of 1:20,000 is preferable. After the stain has acted for ten minutes it should be washed off with saline solution (0.6 per cent. NaCl in distilled water), and the preparation covered with this solution.

Two regions of the nephridium stain heavily with the dye—the nephrostome and the ampulla. Within wide limits the more dilute the stain the greater the contrast between the amount of dye absorbed by these regions and that absorbed by the remainder of the nephridium. Since the stained nephrostome is clearly visible, it is not difficult to remove the entire nephridium for study under the microscope. In the more dilute solutions the cytoplasm of the central cell, as well as the cytoplasm of the marginal ciliated cells, is strongly stained. The nature of the ciliary action and the direction of the effective beat can be clearly made out. In more concentrated solutions ciliary action is likely to cease, concurrent with the staining of nuclei of the cells of the nephrostome.

The ampulla is stained distinctly in solutions of the concentrations mentioned above, due to the accumulation of the dye within the cells.[2] The outer portion of the ampulla does not betray its cellular nature in dilute solutions, but in strong concentrations cell nuclei are clearly delineated. The inner portion of the ampulla stains strongly in any case. This area, said by Maziarski[3] to consist of rod-shaped bacteria packed closely together, is so strongly stained that when examined macroscopically it may be mistaken for the nephrostome. Its position at the distal part of the long loop of the nephridium serves to distinguish it from the nephrostome, which lies much nearer the median ventral axis.

Complete nephridia, strongly stained, may readily be removed, dehydrated rapidly in absolute alcohol, cleared in xylene, and mounted in balsam. If desired, to retain the dye fully, such preparations may be fixed in ammonium molybdate and washed before dehydration. Such mounts usually show fine detail, together with unusual translucency, and are therefore well adapted for careful study.

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SPECIAL ARTICLES

ALTERNATING CURRENT CONDUCTANCE AND DIRECT CURRENT EXCITATION OF NERVE

THE Fourier integral has proved to be a powerful and useful tool in many branches of science. In the Heaviside operational calculus form it has been particularly valuable in studying the transient behavior of electric circuits. When certain simplifying assumptions and approximations are made, this type of analysis points out a relation between the alternating current conductance and the direct current excitation of irritable biological tissues.

Alternating current resistance and capacity measurements over a wide frequency range show that biological materials may be considered electrically equivalent to a circuit containing two fixed resistances and a polarization element having an infinite impedance at zero frequency and a zero impedance at infinite frequency. This element may be considered as a resistance and a capacity in series, both of which decrease with increasing frequency, n. When $r(\omega)$ is the resistance and $x(\omega)$ is the reactance $(1/C\omega)$ of the capacity, it is often found that $r(\omega) = r_1\omega^{-\alpha}$, $x(\omega) = x_1\omega^{-\alpha}$, where $\omega = 2\pi n$, r_1 and x_1 are the resistance and reactance for $\omega = 1$, and α is a constant between zero and one. The impedance of the element $z(\omega) = z_1 p^{-\alpha}$, where $z_1 = \sqrt{r_1^2 + x_1^2}$, $p = j\omega$, and $j = \sqrt{-1}$, and the phase angle is constant, $\phi = \tan^{-1} x_1/r_1 = \alpha\pi/2$,[1] (1).

When a constant current i is started through this element at time $t = 0$, the potential difference across the element may be found by either the Fourier integral[2] or the operational[3] method to be $e(t) = z_1 i t^{\alpha}/\Gamma(1+\alpha)$, where $\Gamma(1+\alpha)$ is the gamma function. This means that when the equivalent polarization element of a biological tissue has a constant phase angle and an impedance which is a power func-

[2] R. Chambers, "Some Changes in Dyeing Cells," *Proc. Soc. Exper. Biol. and Med.*, 20: 367–368, 1923.

[3] S. Maziarski, "Sur la structure des néphridies des Vers de terre," *C. R. Soc. Biol.*, Paris, Vol. 53, 1901.

[1] H. Fricke, *Phil. Mag.* (7) 14: 310, 1932.

[2] G. A. Campbell and R. M. Foster, "Fourier Integrals for Practical Applications." Bell Telephone System Monograph B–584, New York, 1931. Pair No. 516.

[3] V. Bush, "Operational Circuit Analysis," p. 197, New York, 1929.

tion of the frequency for alternating current, then the potential difference across this element should build up as a power function of the time after a constant current is started through it. The strength of current necessary to change the potential by a fixed value e_0 in the time t is then given by $i = i_1 t^{-a}$(2), where $i_1 = e_0 \Gamma (1+\alpha)/z_1$.

It is reasonable to assume that this polarization element is to be identified with the cell membrane and that a threshold change of potential across the membrane will stimulate an excitable tissue. When it is further assumed that the membrane parameters are constant up to a threshold potential change, then the strength-duration relation for constant current excitation should be given for short times by equation (2).

The fixed resistances of the equivalent tissue circuit may be neglected for stimuli of short duration, but they are important for the longer stimuli and determine the rheobase. In the general case, the superposition[4] formulation and the Heaviside operational method lead to the same asymptotic expansion for e(t). For the simple condenser hypothesis, $\alpha = 1.0$ and the solution is well known. The Nernst diffusion hypothesis corresponds to $\alpha = 0.5$ and the solution may be given in terms of the error function.[5] This theoretical membrane potential change has the general form of the subthreshold direct current excitability curve[6] up to its maximum, but does not give the subsequent decrease, which is found experimentally.

Both alternating current conductance and direct current excitation data on the same preparation are not yet available, but equation (2) has at least qualitative support. Conductance measurements show that the polarization elements often have an approximately constant phase angle for the complete frequency range or at least a major portion of it.[7] Equation (1) gives a value of α from 0.62 to 0.71 for frog sciatic nerve,[8] and 0.73,[7] 0.79[9] for mammalian muscles. Excitation data for short times give a value of α from 0.53 to 0.86 for frog and toad sciatic,[10] and 0.75 for toad sartorius muscle.[11] These data suggest that conductance and excitation phenomena involve the same polarization element and that this element is not represented by either the condenser or Nernst hypotheses in their simple forms.

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[4] The author is very much indebted to Professor H. T. Davis, of Indiana University, for the inversion and expansion of the Volterra integral equation encountered.

[5] Campbell and Foster, *loc. cit.*, Pair No. 551.

[6] G. H. Bishop, *Am. Jour. Physiol.*, 85: 417, 1928; J. Erlanger and E. A. Blair, *ibid.*, 99: 108, 1931.

[7] K. S. Cole, *Jour. Gen. Physiol.*, 15: 641, 1932.

[8] H. Lullies, *Arch. ges. Physiol.*, 221: 296, 1928; R. Labes, *Arch. exp. Path. u. Pharm.*, 168: 521, 1932.

[9] H. Fricke, *Physics*, 1: 106, 1931.

[10] K. Lucas, *Jour. Physiol.*, 35: 105, 1906; L. Lapicque, "L'Excitabilité en Fonction du Temps," pp. 92, 95, 96, Paris, 1926; W. A. H. Rushton, *Jour. Physiol.*, 74: 424, 1932.

[11] K. Lucas, *loc. cit.*, p. 104.

THE EFFECTS OF CIGARETTE SMOKING UPON THE BLOOD SUGAR[1]

THE gratification derived from smoking has always been rather a mystery. Exactly what elements in the smoke exert the pleasurable physiological effects has never been determined, nor precisely what these effects are. Numerous theories have been advanced. But these theories merely show how little is known.

Tobacco differs from other leafy vegetables in its characteristic alkaloid. That alkaloid, nicotine, is named for Jean Nicot, who introduced tobacco chewing to Catherine de Medici. Nicotine is a powerful drug. It paralyzes nerve ganglia when applied directly to them. But it has not been shown—and it is on the whole improbable—that this property of nicotine accounts for the effects of tobacco smoking.

Chemists have pointed to the carbon monoxide in tobacco smoke and have suggested that it is a cause of the ill effects, if not the pleasure, of smoking. But in fact a heavy smoker accumulates less carbon monoxide than does the non-smoker who takes a walk on Fifth Avenue, New York, during the hours of heavy automobile traffic.

Other products of combustion, notably pyridine, have likewise been suggested; but they occur, not only in tobacco smoke, but also in the smoke from other vegetable matter, such as corn silk, maple leaves and coffee beans. That these substances do not contribute appreciably to the gratification of smoking is conclusively demonstrated by the fact that few smokers adhere to the juvenile substitutes for tobacco. Such substitutes are cheap, yet tobacco maintains its popularity. Why tobacco?

The answer we believe is nicotine. Smoking, we find, produces a definite, although temporary, increase in the concentration of blood sugar, and a corresponding increase in the rate of sugar combustion in the body. These effects certainly are due to the nicotine of the tobacco and they arise from the action of this alkaloid upon the adrenal glands. There can be little doubt that this is the source of at least a considerable part of the gratification from smoking.

Our observation of the hyperglycemia from smoking occurred by chance. We had been investigating the question of the optimum mealtime interval—how often should children, college students and industrial workers be fed. To this end we determined the respiratory quotients, at hourly intervals during the day, on several hundred subjects. In a number of cases

[1] From the Laboratory of Applied Physiology, Yale University.

the concentration of sugar in the arterial blood was compared with the respiratory quotient.

On some days the subjects fasted; on others they ate from one to five meals, variously spaced. As was to be expected, the respiratory quotients of the fasting subjects fell to values between .78 and .82 and the blood sugar to .08 and .10 per cent. In the subjects who ate, both the respiratory quotient and the blood sugar rose after the meal; but within 2 to 4 hours, if another meal was not taken, it fell again to the fasting level. When this fasting level was reached, it was maintained in the great majority of the subjects with little change for many hours. A few, however, exhibited sudden fluctuations of considerable magnitude in both the respiratory quotient and blood sugar.

Such fluctuations never occurred among the children. All the adults were free from emotional disturbances which might explain them. A search for the cause of the divergent values suggested that it was associated with smoking.

The respiratory quotients and blood sugars before and after smoking were then studied in a number of subjects. The results showed that when the respiratory quotient is above .85 and the blood sugar correspondingly above .13 per cent., the smoking of a cigarette has no appreciable influence upon either. When, however, the respiratory quotient and blood sugar have fallen below these values, and especially when the fasting level has been reached, the smoking of a cigarette is followed by a rise in both. Values are attained within 15 minutes as high as .85 or .90 for the respiratory quotient and .12 or .14 per cent. for the sugar. During the next 30 minutes the values fall gradually to, or slightly below, those observed before the cigarette was smoked.

It is a well-known fact that injection of nicotine into animals is followed by a temporary rise in blood sugar.[2,3,4] But so far as we can find no one has previously reported a similar rise in man resulting from the nicotine of tobacco smoke.

From animal experimentation it is well established that it is the action of nicotine upon the adrenal glands which leads to the hyperglycemia.[3, 5, 6, 7] The rate of discharge of adrenalin is increased; and the liberated adrenalin exerts its characteristic glycolytic action. The glycogen stored in the liver and muscles is converted into sugar. In consequence the concentration of sugar in the blood is increased. Secondary to the rise in sugar the combustion of carbohydrate is increased and can be observed in the increase in the value of the respiratory quotient. But, as already stated, these metabolic effects do not result from smoking when the blood sugar is at a concentration above .13 per cent. as it is for 2 to 3 hours following a meal.

The acceleration of sugar metabolism thus demonstrated affords a possible explanation for the fact that smoking diminishes hunger in many users of tobacco. Hunger appears from our observations and those of other investigators to arise within a definite time after the blood sugar falls to the fasting level. Tobacco smoking, by inducing a hyperglycemia, may thus delay temporarily the development of hunger.

Our observations on the mealtime intervals indicate that the hyperglycemia following a meal definitely relieves the fatigue and irritability that generally develop soon after the fasting level of blood sugar is reached. Smoking by inducing a hyperglycemia temporarily relieves these conditions.

The other effects of smoking, the acceleration of the pulse and the temporary rise in arterial pressure, are presumably, like the increase in sugar concentration, dependent upon discharge of adrenalin.

Our observations demonstrate why tobacco rather than any other substance is used for smoking; the smoker obtains from tobacco repeated minute doses of nicotine.

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